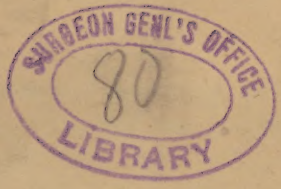
SMITH (N.R.)

Supplement to an Essay on Digestion.



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SUPPLEMENT

TO AN

ESSAY ON DIGESTION.

BY NATHAN R. SMITH, M. D.

PROFESSOR OF ANATOMY AND PHYSIOLOGY IN THE JEFFERSON MEDICAL COLLEGE, PHILADELPHIA.

In a recent attempt at something, on the subject of digestion, more conformable than the present theory, to opinions now entertained in relation to life, and to the principles which have obtained in other branches of science, I did not promise myself, or the public, more than to call their attention to some important defects, in the principles of the hypothesis which is so generally and tacitly received.* At the same time, however, I did not despair of being able to suggest something, not heretofore advanced, which should, in part, supply the deficiency that might result from successfully maintaining the above objections. Although I may have manifested a degree of earnestness in advocating my opinions, I trust that my remarks no where betray a confidence that I should at once overthrow that, which has been the work of so many hands, and upon which rests the property of so many speculators in physiology and pathology. Such being my anticipations, therefore, the reception, with which the essay has

* See Preface to the Essay.

Repr. from: Am. M. Rev., Phila., 1825, II, 105–136.

met, can not but be highly gratifying. That it should be deemed worthy of consideration, in opposition to the mass of opinion which sanctions the present system, is the highest degree of immediate success which can be anticipated for an attempt of this kind. But I know that, in the minds of many persons of discernment, the circumstances which are there concentrated upon this subject, have given rise to a train of inquisitive reflection that has been by no means unfavourable to the opinions which I have endeavoured to substantiate.

They have been led to appreciate the inadequacy of the mode of reasoning, which has been generally employed in relation to the functions of life, and which was alluded to in the following words:

"When we confine our inquiries to any particular science, with the laws of which we are acquainted, and reason from an effect to a cause, which in its operations we know to be controlled by those laws, there need be no fallacy. But if we enter another field of science, the laws of which we have not investigated, and reason from an effect to a cause, we shall almost certainly fall into an error, for we unconsciously attribute the effect to a cause with the operations of which we are familiar. Thus, if we combine two inorganic substances, and after a time, observe that a remarkable change has taken place in their properties, we at once, and with propriety, attribute it to the influence of chemical affinities; for we know that such substances are exclusively under the control of physical properties. But if we convey certain substances into the stomach of an animal, a polypus for instance, and upon removing them after a considerable time, discover that they have become very much changed, having received new chemical and sensible properties, it is altogether unwarrantable to infer that these changes are wholly the effect of chemical influence; for, as the stomach of this and of every other animal is indued with certain vital properties, and as we are not yet fully acquainted with their laws or uniform habits of action, we can not say how much, in this instance, is to be attributed to their influence.

"That similar effects, then, in any particular science are referrible to the same cause, is generally true; but that similar effects in two distinct sciences, the cause being known in one, are referrible to the same cause, is, in principle, false."

And again, "Experiments upon living animals, with a view to multiplying facts in relation to life, appear to characterise the researches of the present day. That these under some circumstances, and performed with proper precaution may yield useful matter, is undeniable; but it is necessary to their success that we should, to a certain extent, be acquainted with the properties of life, and laws by which the exertion of them is governed; otherwise, we do not know what effects they may be capable of accomplishing, and are in danger of attributing the results of our experiments to causes with which we are more familiar. The little progress which has been made in physiology by the multitude of these experiments, confirms what I have stated. We perceive that experimentors, who may even essentially differ in opinion, find no difficulty in substantiating their favourite notions, by the same series of experiments."

The equivocal character of many of the experiments which have been relied upon, and their inconsistency with each other; the extreme difficulty of demonstrating any solvent powers in the gastric liquor out of the stomach, and even the absolute impossibility of doing it, in the hands of some of the ablest chemists, who were, nevertheless, believers in its agency; the equivocal tendency of the post mortem erosions of the stomach; the inconsistency of the experiments of W. Philip with the common hypothesis, irresistibly proving, by the immediate suspension of digestion which follows the division of the eighth pair of nerves, that there is something beyond chemical agency; the same effects being produced by any sudden emotion of the mind, or other cause which may be supposed to divert nervous influence from the stomach, are all circumstances which we know have been appreciated as of no small weight.

The supposition, too, that so important a change is so *rapidly* wrought, and upon so large a volume of matter, must have appeared inconsistent with the analogy of other assimilative processes. The want of uniform characteristic properties in the chyme, and the fact that chyle resembling that of the lacteals is not found in the intestines, I believe have had

their weight, as have also the objections drawn from the anatomy of organs concerned in digestion, the inadequacy of the thoracic duct to the performance of the functions attributed to it by the theory in question, the redundancy in the magnitude and structure of the liver, and finally the want of any peculiarity of structure in the mucous membrane of the stomach, corresponding to the peculiar and extraordinary function attributed to it, viz. the secretion of the gastric juice.

I cannot but flatter myself, from the opinions which have been expressed, that the circumstances brought together, as warranting my position that a portion of the aliment is taken up by the extreme veins and in them prepared for a still greater change in the capillary system of the liver, have been favourably appreciated. Among these it will be recollected, by those who have perused the essay, are, first, the fact that the absorption of *some* substances by the veins, is unquestionably performed, as proved by Majendie and others; second, the effects of the division of the eighth pair of nerves; third, the analogy between the digestion of plants and of animals; fourth, the digestion of some of the lower orders of animals, in which nothing like gastric juice can be detected; fifth, the fact that all the subsequent stages of assimilation are effected in capillary vessels, as well as all the other changes which are wrought upon the circulating fluids; sixth, the circulation of the vena portæ, which hitherto has not been satisfactorily accounted for; seventh, the structure and anatomical relations of the liver, which, by our hypothesis, is redeemed from its comparative physiological insignificance; eighth, the phenomena of the fœtal circulation, and the analogy existing between the nourishment of the fœtus by the veins of the placenta, and nourishment after birth by the veins of the stomach and intestines; these two systems of capillaries being opposite extremes of the same continuous vessel, the vena portæ, from one extreme of which to the other I suppose the function of digestion to be transferred at the time of birth.

I could not flatter myself, however, even if I were so for-

tunate as to have adopted the explanation of these phenomena, which is, in the main, true, that it was at once wrought into an unobjectionable shape, or undeformed by error. The circumstances under which the materials of the essay were brought together; the limited opportunities which I enjoyed of intercourse with enlightened members of the profession, and of access to the original memorials on this subject, a constant routine of laborious country practice, and, in winter, the responsibility of a course of lectures on anatomy, without even an assistant in my dissecting, would have precluded this, though the nature of the subject were susceptible of it. Subsequent impartial inquiry, however, has confirmed my confidence in the general correctness of the principles advanced. At the same time I frankly acknowledge, that there are some important deficiences, which resulted from my not having properly appreciated certain circumstances hostile to my opinions and friendly to the theory of the gastric juice. On examining the details of the experiments in relation to this subject, performed by Spallanzani, Gosse and Stevens, I am satisfied that there is a greater change wrought upon the contents of the stomach, through the *medium* of the fluids contained in it, than I had been led to suppose. I trust, however, to make it clearly appear, that this change is not wrought by the chemical action of a fluid, possessing the remarkable properties attributed to the gastric juice; but that the secretions of the stomach are not necessarily remarkably different from the saliva, or the mucous secretions of other parts. In the essay I hinted that the aliment might be, before absorption, resolved, by the *powers of life*, into its proximate or ultimate principles. None of my positions are inconsistent with such an hypothesis. On the contrary, the mode of reasoning there adopted, and the principles advanced, although they are incompatible with the idea of a chemical solution taking place in the stomach, nevertheless, harmonise admirably with the supposition that this may be effected, in some way, by the vital powers. That I have sub-

scribed to nothing which would preclude inquiry into the changes which life may effect upon the aliment, while yet in the stomach; and that the logic, which I employed, left me completely at liberty to prosecute such an investigation, and even prompted to it, is apparent from the following:

"We cannot, by them, accurately determine, how much is the effect of physical, and how much of vital properties, until we know something more of the powers of the *latter*. It would, therefore, be more profitable, if we were to direct our attention to inquiring what degree of control *they* exert over the phenomena of life, rather than to inquiring how much influence the chemical affinities have over the same."

Perhaps it is already anticipated, that I would attribute the effects, which are produced on the aliment in the stomach, preparatory to absorption, to the nervous influence of that organ, using the exhaled fluids as merely a medium of transmission.

The vague conjectures which were thrown out by some of the earlier physiologists concerning the subtile fluid, which they supposed to pervade every part of the nervous system and to serve as the missive agent of volition and sensation, are to be regarded as the result of a habit of thought, unconsciously acquired by observation of natural phenomena, by which it becomes almost impossible to conceive of the relation of cause and effect, between two substances, without presuming some material intercourse. Nothing, however, analogous to this imperceptible principle, had yet been discovered in physics; hence, when, by the employment of the inductive philosophy, the mode of reasoning on these subjects became more rigid, the idea of a nervous fluid, or any other subtile agent, which could neither be felt nor seen, was regarded as an unwarrantable assumption. It is a little remarkable, however, that even the fastidiousness of the inductive philosophy could not reject language of similar import, though more abstracted. It has never been considered illogical to speak of the transmission of nervous influence; but

who can use this language, without conceiving an idea of the passage of something material? Experimental philosophy itself, however, soon detected in physics, subtile principles, corresponding to the nervous fluid of life. Aeriform elementary bodies were evolved, imperceptible to most of our senses, and rather characterised by the effects which they produced upon other and more palpable substances, than by any sensible properties of their own.

The discovery of the electric principle, so obscure in its nature but so remarkable in its effects, was a momentous intellectual achievement in this department of science. The galvanic modification of this principle had accidentally been discovered to have a remarkable influence over the organs of life. From certain effects produced by it, upon the muscles of animals recently dead, Galvani and others were led to suspect the identity of this fluid with the agent of nervous influence, or, at least, its resemblance to it. This *rationally* revived idea of the existence of a nervous fluid, supported by so strong an analogy, and even by experiment, would, at that time, have been more regarded, had it not shared the odium which had fallen upon the unfounded speculations of the earlier writers on this subject.

It was not till recently, that the experiments of Wilson Philip and others, compelled the more particular attention of men of science to this subject; employing the inductive philosophy to establish, upon the imperishable basis of fact, an hypothesis, which it once properly rejected, because, then, it was neither an axiom nor an induction from the analogy of the collateral sciences.

The experiments, which are chiefly relied upon, as proving the identity of the nervous influence with the galvanic fluid, or, as is only conceded by most physiologists, its resemblance to it, are, first, the application of the latter agent to the nerves of animals after their connection with the brain has been destroyed, or immediately after death, before the muscles may be supposed to have lost their susceptibility of contrac-

tion, and which is found to produce powerful contractions precisely similar to those effected by the agent transmitted from the will. The same effect is not produced by any other extraneous agent. From this absolute *identity* of result, we may, at least, safely infer a *similarity* of cause.

Second, the substitution of the galvanic fluid for the nervous influence in the functions of the stomach and lungs, as was apparently effected by Philip. To accomplish this, he first divided the eighth pair of nerves, and on comparing the result with the ordinary phenomena of digestion, he found that the process of assimilation was completely and immediately suspended. He then connected one pole of the galvanic battery, with the stomachic extremity of the divided nerve, the other with the region of the stomach, and immediately the functions of the organ were resumed. From comparing this condition of the stomach, with that in which the galvanic battery was not employed, and also with that in which the nerves were not divided, he satisfied himself of the sufficiency of the galvanic fluid, as a substitute for nervous influence, and hence inferred their identity. He supposes, however, that it is only indirectly that the nervous and the galvanic fluids influence digestion, by promoting the secretion of the gastric juice. A little reflection must, as I think, convince us that this inference is by no means necessary, nor indeed warrantable. He does not arrive at this conclusion, from having actually seen the secretion of the gastric juice to be suspended by the division of the nerves; not from having compared the quality and properties of the fluids secreted, in both cases; but from the *presumption* that the function of digestion can be affected by it in no other way. Such a conclusion is incorrect, because, not fully knowing the nature and influence of the vital powers, he does not know but that the interruption of them may influence digestion in some other way, even although we grant that the gastric juice is essential to digestion.

The above inference of Mr. Philip, obviously rendered it

necessary for him to presume the indispensibility, every where, of the nervous fluid to secretion. It is here that Mr. Philip's doctrine is vulnerable, and here it has been successfully attacked by Mr. Brodie. This gentleman has proved by a series of experiments, and in the most satisfactory manner, that the nerves are no more essential to secretion than they are to circulation. Mr. Brodie, I know, goes still further, and infers the fallacy of Mr. Philip's observations, in regard to the interruption of the digestive process, by the division of the gastric nerves. The statement of the latter gentleman, however, in relation to the *final* effect witnessed by him, is so substantiated that it can not well be denied, and although some of Mr. Brodie's experiments may appear, at first view, to render his results equivocal; yet Mr. Philip has in reply, as I think, satisfactorily pointed out certain circumstances, necessary to a complete interruption of the nervous influence, which were not fully attended to by Mr. Brodie. Besides, we have, in confirmation of Mr. Philip's observations, certain natural phenomena, which bear a strong analogy to the effects supposed to be produced by the division of the nerves. Sudden and deep emotions of the mind, violent exercise, the infliction of extreme pain, causes which make a powerful impression upon the nervous system, and therefore may be supposed to divert nervous influence from this organ, are found to interrupt at once the digestive process. Mr. Philip's statement, therefore, with regard to the final effect which results from the division of the nerves, is probably correct; but there is another circumstance, which contributes to invalidate his supposition that it is accomplished by interrupting the gastric secretions. We are compelled to admit, that the change which the food undergoes in the stomach, may take place remotely from the *surface* of the organ, where the fluid in question is exhaled. We are compelled to admit, too, that it will even take place in hollow balls, perforated with small holes, through which the supposed solvent fluid can only get access slowly. Hence it is certain that it must

take place simultaneously through the whole mass, by the medium of the fluid which has been for some time accumulating. Neither Mr. Philip, nor any one, who advocates the theory of solution by the gastric fluid, will deny that this fluid does accumulate in the stomach. Indeed, all their arguments are either built upon, or imply the presumption that it often exists, in quantity, in that organ. This being the case, then, Mr. Philip's supposition, that the division of the nerves interrupts digestion only by suppressing the secretion, is obviously inadequate. He states that digestion *immediately* ceases after the division, and we know too, that the same effect immediately follows emotions of the minds, but if the effect results only from the suppression of the secretion, it is obvious that the solution ought not to be *immediately* suspended. If, after the secretion of the fluid, the process is chemical, as is stated, it surely ought to be continued, at least for a time, although all vital influence be withdrawn.

The above experiments, therefore, and the natural phenomena which we have mentioned, irresistibly prove, that the nerves have *directly* an important influence upon the process of digestion. But shall we say with Wilson Philip, that it is through the agency of a fluid identified with the principle of galvanism? If the subject be viewed with candor and in its proper light, I think that even this position can not be regarded as absurb. It is unphilosophical to reproach the advocates of such an opinion, with attempting to pry into the forbidden secrets of nature, and of wasting their intellectual efforts upon that which is unattainable. Let it not be supposed that they are attempting to develope the essential nature of matter, of causation, or of life; but merely to interpose other links in the chain of sequent causes and effects.

When we look back, and trace the progress of the mind to the present state of our knowledge, when we see it following the flight of comets, weighing the sun in a balance, measuring the velocity of light and imprisoning the lightnings of heaven; when we see it dividing and subdividing the ancient

elements into subtile principles, of the nature of which no anticipation could have been formed, and, at each step, evolving new agents which enable it to act still more minutely on the atoms of matter, what presumption is it not to determine what it shall be unable to accomplish hereafter? The innate propensity to inquiry, which we possess, will not suffer us to remain satisfied with our acquisitions, while there exists any circumstance, that may lead us to a more intimate acquaintance with the secret agents, by which matter acts upon matter. The mind should not be too much hampered with definitive rules, which, as our knowledge advances, may be found altogether inadequate to its powers; nor should it be hedged in and limited in its sphere, lest it encroach upon venerable dogmas. *Truth can never be hostile to the interests of man, nor to the dignity of human nature.*

Besides that we have the experiments and the opinions of one of the most logical physiologists of the present day, and these confirmed by others, there are further reasons why the identity of the nervous and galvanic fluids is not to be regarded as an absurdity. It is certainly not improbable, that *animal life* may employ agents equally sublimated with those which pervade inanimate matter, and which confer upon that its physical constitution or *material vitality*. Not improbable? is it not a *fact* beyond the reach of sophistry, that in some instances, at least, the electric fluid is one of the instruments of life, and that the employment of it is under the controul of the nervous system, as in the torpedo and electric eel? The voluntary phosphorescence of the fire-fly, and other insects, is equally illustrative of our principle.

It is not necessary, however, that I should further vindicate the opinions of those who have assumed the above position. I only claim that the two fluids bear a strong *analogy* to each other, in their mode of operation and in their effects. In this I am supported by the observation and opinions of nearly all the physiologists of the present day. Cooke, in a

very learned work on the physiology and pathology of the nervous system, observes as follows:

"That nervous influence may depend on galvanism, or some other modification of electricity, has been lately, with several physiologists, a favourite hypothesis. The subtilty of the galvanic power, the velocity of its motion, its action on muscles through the nerves, and the means of diminishing, extinguishing,* and renewing the susceptibility of animals to its influence, are circumstances alleged in support of this doctrine. Professor Aldini, from his experiments, infers that a peculiar etherial fluid is continually generated in the animal economy; that it is connected with the functions of life; and that as there is a metallic pile, composed of metals and fluids, so there is also an animal pile, consisting of living animal substances. Dr. Valli remarks, that substances which conduct electricity, are likewise conductors of the nervous fluid; and substances which are not conductors of electricity, are not conductors of the nervous fluid.

"Mr. Good, after adverting to the opinion that oxygen may be the cause of fibrous irritability, and observing that it was never conceived altogether competent to the production of muscular motion and sensation, says: 'Hence other gases have alternately been glanced at, as affording the chief forms of this recondite and attenuate power. Electricity has been principally studied with this express view; and the study has, at length, been crowned with a success, which, though by no means perfect, opens to us, perhaps, the way to perfection. In consequence of the experiments of Cotugno, Vassali-Eandi, Galvani, Volta, and many other celebrated philosophers, it has ultimately been demonstrated that animals are capable of generating or exciting an electric aura in their own bodies, as well as of receiving it from without; that this electric aura is possessed of all, or nearly all, the properties of common or metallic electricity.† The mode by which the body, or rather the nerves, become possessed of this mysterious and truly spiritual aura, is still doubtful. That it is rather received from the surrounding atmosphere than communicated by our food, is, I believe, generally admitted; and hence, like caloric

* Both the galvanic and nervous power are destroyed by an exposure to sulphuretted hydrogen gas, carbonic vapours, &c. See account of experiments on galvanism, in the supplement to the Encyclopædia Britannica, vol. i. p. 681.

† Good, vol. i. p. 412.

and oxygen, it commonly enters into the system in the act of respiration, and forms a constituent part of the calor ventusque vitalis of the poet.* Fishes, he adds, absorb atmospheric air in general, not by the lungs, for they have none, but by gills, which answer their purpose; and several of them, as the torpedo, gymnotus electricus, and silurus. secern the galvanic aura, of which it contains the basis, not by the brain, but by an organ which enables them to secern it in a much larger quantity, and at will; an organ which is a natural voltaic pile, and like the voltaic pile, enables them to communicate it, in an aggregate state, to other animals, upon contact, in the mode of sensible and often very severe shocks '†

"Speaking of the galvanic and electric auras, which he considers as the same, Mr. Good says: 'It is almost reduced to a certainty, that this common aura constitutes the nervous fluid; and, in man, at least, it should seem therefore to be secreted from the brain, and hence diffused over the body by the course of the nerves.'‡

"Mr. Carlisle, in a communication with which he has favoured me respecting the nature of the nervous power, says: 'The facts elicited by galvanic discoveries, added to the former proofs of electrical discharges from the nervous systems of the gymnotus§ and torpedo, and these genuine electrical sparks being clearly issued at the voluntary discretion of those animals, formed a series of leading points, very promising of some improvements in the knowledge of the brain and nerves; but no such result has yet occurred which might become applicable to the healing art. I have long entertained suspicions that both animal volition, and the more remarkable secretory changes, are governed by electricity, and have ventured to mention those conjectures in the two Croonian Lectures, printed in the Transactions of the Royal Society of London, for the years 1804 and 1805; but any conclusive inductions to be drawn from such intricate and multifarious phenomena, require more extensive researches and more undivided attention than I am able to bestow."

Good remarks as follows: "Whatever be the nature of the

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* Lucretius. † Good, vol. i. p. 413. ‡ Ibid, p. 119.

§ "A particular adaptation for the nerves which supply the electrical batteries of the torpedo and gymnotus, Mr. Carlisle says, is observable on the exit of each from the scull; over which there is a firm cartillage acting as a yoke, with a muscle affixed to it, for the obvious purpose of compression; so that a voluntary muscle probably governs the operations of the battery."—*Phil. Trans.* 1805. p. 11.

active and etherial fluid which was thus traced by Stuart, and has since been fully established by Galvani, there can be no question of its having a powerful influence upon many branches or divisions of the nervous system, though not upon all. Its effects upon the muscles of an animal for some hours after death are too well known to be particularized: and Dr. Philip seems to have shown, by various trains of experiments,* that it is equally capable of maintaining respiration, and the operation of several of the animal secretions, especially those that induce digestion, for as long a period. But in drawing from such facts the corollary that the 'IDENTITY of galvanic electricity and nervous influence is established by these experiments;' he seems to proceed farther than he is warranted: for we have no right to say more, than that galvanic electricity is a stimulus exciting the nervous influence into a state of continued secretion, or continued action; which may possibly be done by various other stimuli, as well as by that of galvanism."

And again: "This, however, is not mere conjecture: for in different ramifications of the nerves, we can trace such different effects actually produced; and as it has sufficiently appeared that the operative power is a quick and subtle fluid, we are directly led to conclude that such difference of effects must depend on a diversity of fluids, or on various modifications of a common fluid in different ramifications: the last of which explanations is by far the simplest and easiest."

Mr. Abernethy has said, that he considers it already demonstrated that they are electrical actions, which cause the various combinations and decompositions, so constantly occurring in all parts of the body. Sir Everard Home expresses a similar opinion.

Some recent writers have suggested, that the nervous fluid is electricity brought under the control of life, and divested of some of its properties; becoming, what they denominate animal electricity. There is, indeed, something peculiar in the electricity of the torpedo and the gymnotus, since it has been ascertained that it can not be conveyed by a metallic conductor, nor will influence the most delicate electrometer. These

* Phil. Trans. 1815. p. 5—90.

are circumstances which explain the failure of a great many of those experiments by which it has been attempted to demonstrate, unequivocally, the presence of this fluid in the system.

It is now, I believe, well ascertained and generally admitted, that chemical affinity is merely a modification of the electric principle. That chemical compositions and decompositions are greatly controlled by it, and that its presence renders those substances active agents, which before were apparently inert, no one will deny. For the sake, however, of illustrating the analogous offices of the nervous fluid, or animal electricity, I will quote an experiment of Sir Humphrey Davy.

"A few globules of mercury are placed in a vessel containing common pump water; or any water that contains a small quantity of saline impregnation; wires from a battery of one thousand double plates, not very strongly charged, are introduced into the vessel opposite to each other, so as to reach the bottom; as soon as the circle is completed, the mercury will be violently agitated, each globule will become elongated towards the positive pole, but will retain its circular outline in the part opposite to the negative pole; oxide will be given off from this part, which is positive, but no hydrogene from the part which is negative, and the oxide will pass in a rapid current from the positive towards the negative pole. As long as no hydrogene is given off, the globule is in continued agitation, and a stream of oxide flows with great rapidity from the positive to the negative surfaces; and the negative surfaces of the mercury approach rapidly towards the positive, which are at rest; if the conducting power of the water is exalted by the addition of more of the saline impregnation, or if the charge of the battery be increased, hydrogene will be given off from the negative poles; and the instant this happens the globules become stationary; as if the same power which gave motion to the mercury was neutralized by, or employed in, the evolution of the hydrogene. There are many other remarkable phenomena connected with the operation of electricity on mercury, in contact with water; which may be urged in favour of the idea, that chemical and electrical attraction depend upon the same cause, and which will possibly lead to new views respecting the ele-

ments of matter; but the consideration of them properly belongs to a more advanced division of this work."

Since, then, between the nervous influence of life and the electric principle of matter, there is such an analogy as almost amounts to identity, and since it is absolutely certain that the electric fluid is capable of establishing an active relation between substances, which before were perfectly inert with respect to each other, giving to water, for instance, the power of acting upon, and dissolving some of the most insoluble substances; is it not rational to suppose, that the nervous influence of the stomach, may bear the same relation to the exhaled fluids of that organ, in regard to the solution of its contents, that the electric influence does to the pump water, in regard to its action upon the mercury, in the above experiment? Is it not more rational to suppose, that a fluid contained in the stomach, possessing no remarkable chemical properties inherent in its own constitution, may serve as a medium, through which the vital agent, whatever it may be, acts upon, decomposes, and recombines the various alimentary substances; than to suppose that these changes are effected, by the immediate chemical influence of a fluid, which is certainly a perfect anomaly among chemical agents?

The question is not, whether one hypothesis be utterly absurb, and the other palpably demonstrable, but, rather, which has the greatest weight of evidence in its favour; and which best accords with the phenomena manifested by the function of digestion, with the analogy of the other functions, the laws of life and those of inanimate matter.

Let us observe, then, how our hypothesis accords with the experimental and natural phenomena of digestion.

It reconciles at once the before incompatible experiments of Philip and Brodie, for upon our supposition, it is obviously no longer necessary to maintain, that the nervous influence is necessary to secretion, in order to account for the effect of dividing the nerves of the stomach upon the functions of that

organ. It also accounts, satisfactorily, for the *immediate* effect of those experiments which, the reader will recollect, we found altogether inconsistent with the idea, that they merely interrupted the secretion of gastric fluid; and also for the *immediate* influence of emotions of the mind, pain, &c. which may be supposed to counter-excite other parts of the nervous system, and to concentrate its energies, in some intense local action of health or disease.

Our supposition also explains away the anomaly, which the former theory must admit, viz. the existence of unparalelled chemical energy in a fluid, which has nothing remarkable in its sensible properties, or chemical constitution, and which appears to possess none of those principles, which are so remarkable in other (I will not say equal, for there are none, but inferior) chemical agents. But the former hypothesis, presumes not only an unequalled *energy* of action; it necessarily supposes the gastric juice to be a very *general* agent, influencing a greater variety of substances than any other fluid with which we are acquainted. The galvanic principle, however, to which we liken the nervous influence, is capable of conferring a most energetic solvent power upon the most inert fluids, and through their medium of dissevering those substances which are most tenacious of each other, and forcing them into reluctant union with others. The believers in the gastric juice have had so much difficulty in accounting for the general solvent power of this fluid, that some have absurdly presumed the peculiar stimulus of each kind of food to excite the secretion of a peculiar and appropriate kind of gastric juice. Were this true, the stomachs of many persons would, at each meal, be taxed for at least half a score of these distinct solvents.

That the stomach is not irritated by the presence of so powerful a chemical agent, as the gastric juice is supposed to be, has been promptly accounted for, by attributing to it a peculiar sensibility adapted to the nature of this fluid. But how shall we account for its affecting so slightly the organs

of taste, or for its comparative inertness when injected into the bladder, as was done by Dorsey.

"A silver catheter was introduced into the bladder, and to its external extremity was adapted a hog's bladder. About two ounces of gastric liquor from a hog's stomach, were injected into the bladder, through the catheter; the patient declared it gave him no pain but from the catheter having scratched the urethra. A proof that he felt no pain was, that he was unconscious when the fluid entered his bladder. Sometimes, in subsequent experiments, he distinguished it, but most commonly he did not. After remaining about three hours in his bladder he discharged sixteen ounces of fluid, very turbid. The experiment was made at 4 P. M. In the night he had a chill succeeded by a slight fever, but did not attribute it to the injection."

The membrane lining this organ, is delicately sensible to all those chemical agents, which are capable of acting thus rapidly upon alimentary substances, and if the gastric juice is of this character, the vital properties of these organs should no more protect them against its action, than against that of any other active solvent. How shall we account, too, upon the common principles, for the protracted existence of animals in the stomach, in contact with an agent, to the impression of which they are unaccustomed? Leeches have inhabited the stomach for a considerable time with so little inconvenience as to grow to an enormous size. The advocates of the theory of chemical solution have a ready answer, that the powers of life resist its action; but do the powers of life resist the action of analogous chemical fluids? Are the living solids insensible to those agents which almost instantaneously corrode them when dead? Will life protect the skin against the action of nitric or sulphuric acid? or will a leech live and flourish in them, by virtue of his talisman of vitality? So far from it, we know that the weakest of those fluids, which are remarkable for any energy of chemical action, are immediately fatal to all such animals as are not accustomed to their contact.

It appears to me, that our supposition is the only one which can afford an adequate solution of this difficulty. Granting

that the gastric fluid is in itself bland, and not possessed of any remarkable chemical properties, but that it is the conductor of something like the galvanic principle, which being under the control of life, is capable of effecting a kind of animal solution of alimentary substances; and we have no longer any difficulty. Upon this supposition, it is obvious that *life* may repel those agents which are rendered active solely by the powers of *life*.

In the essay, I did not attempt to account completely for the erosions of the stomach which are observed occasionally to take place after death. I endeavoured, however, to show, that they were as consistent with the opinions which I advanced, as with those commonly received. I there noticed the fact, that in those cases, in which erosion of the stomach had been observed, the food contained in the organ was found unaffected. I noticed also the inconsistency of the supposition that it is affected by the gastric juice, with other parts of the theory of solution. If it act upon the stomach after death, it is absolute evidence, that it must have accumulated in considerable quantity; a circumstance which, upon the former theory, as we have already shown, is altogether inconsistent with the sudden interruption of digestion, effected by the division of the nerves, and by other causes which disturb their functions.

I presumed, too, that if a certain degree of heat and motion are the only circumstances which aid solution by the gastric juice, and if after death, when these circumstances no longer exist, it produces such remarkable effects, they ought to be unequivocally demonstrable out of the stomach, and not so obscure, as that the ablest chemists and physiologists, as Reaumur, Montegre and Magendie, deny altogether that they take place when it is removed from the organ. It is evident, then, that there is more lost than gained to the advocates of the solvent theory by the admission of the above facts. If, then, I am myself unable to account for them, they are at least equivocal, and neither to be relied upon by one theory nor the

other. But if there exists the *neuro-electric* influence, of which I have spoken, we are certainly afforded greater latitude for their explanation. As observed in the essay, we know that many of the organic functions are continued after apparent death. On opening the abdomen of an animal recently killed, we find the peristaltic motion still continued, and as vigorous as during life. If then, the stomach loses its organic life by slow degrees, and if different portions of it successively die, as is the case in every part of the system, it is obvious that, wherever the dead portion is in contact with the living, it will be subjected to the impression of the nervous influence, and if life reluctantly abandons the stomach, as in cases of sudden death in the full vigor of its powers, this effect will take place in a very considerable degree, following the retrocession of life along the blood-vessels and nerves. This explanation accords well with the fact, that these erosions usually occur near the centre of the great arch of the stomach, where it has least vascularity, and where its organic powers may be supposed to be the most feeble; and also with the fact that it usually occurs in circumscribed spots, and does not affect the general surface of the stomach, as we should anticipate from the diffusion of such an agent through the organ.

It may be said, indeed, that the above explanation is inconsistent with facts upon which I myself rely in other parts of my argument, since I have asserted that a much less efficient cause than the complete suspension of the animal functions, as merely an emotion of the mind, may interrupt the office of the gastric nerves. In anticipation of this, I would observe, that in the latter case, the effect is rationally to be attributed to a concentration of the nervous energy in the sensorial functions; but when the organic powers continue to be exercised after the cessation of the animal functions, whatever nervous energy remains is concentrated in the last rallying points of organic life.

The uniformity of structure in the different portions of the

mucous membrane, lining the digestive organs, is a circumstance which strongly opposes the idea, that there belongs to that part of it, pertaining to the stomach, a peculiar and remarkable office. It is a principle often employed by Bichat, that similarity of structure indicates similarity of function, and, *vice versa*, that similarity of function indicates sameness of structure. But, surely, if the membrane of the stomach is destined to the secretion of the gastric juice, the most remarkable product of secretion met with in any part of the system, and if the principal office of the intestinal portion be absorption, we have a surprising anomaly in physiology. Ought we not upon this supposition, to detect in the gastric portion of the membrane some peculiar structure, superadded for the performance of this peculiar and remarkable function. The only difference, however, that we can discern between the structure of the membrane of the stomach and that of the intestines is, that the former is of a more delicate tissue, more vascular and nervous, and hence, we should presume *a priori*, qualified to perform the same function with greater energy.

The mucous membranes must be regarded as an exceedingly interesting medium of reciprocal communication between organized and unorganized matter. Through them, animate and inanimate matter are constantly interchanging their elements. For this reason they every where present their free surface to the contact of those substances, which bear an important relation to the organized system; one portion of them, the excretions, having just been rejected by the organs of which they formed a part, and divested of the properties of life; while the other portion, consisting of alimentary substances, is just coming under their control, to constitute a part of the organized system. This is true, not only with regard to that portion of these membranes which lines the alimentary canal, but is also admirably so with regard to that of the lungs. It is now generally admitted that the air inhaled, not only becomes the vehicle which removes recrementitious matters from the blood, but that it also imparts

a necessary principle to it, through the medium of the pulmonary membrane.

It has been suggested by a friend, as a circumstance operating against that part of my system, which relates to the absorption of aliment, that, since it is ascertained that nature has provided one route for the passage of pabulum into the mass of the blood, and since she accomplishes her ends by the simplest means, it would be a matter of supererogation to furnish another. Were it satisfactorily ascertained, that the lacteals *do* convey *all the constituent principles* of the blood, and that the route of the thoracic duct is sufficient, under all circumstances, for the performance of the office attributed to it, the objection would present considerable difficulty. But it appears by no means certain, that the different constituent principles of the blood, *can* be elaborated by one chemical process, or further assimilated by one set of vessels. But, as analogy furnishes the above objections, we will resort to it for a reply. The two great classes of organic phenomena are, first, those which are presented in the process of assimilation, or the enduing of aliment with the vital properties of the blood, and second, those which occur in the secerning processes, or the regression of matter from its vital to its physical condition. The functions which constitute the latter class, and which are the least important, we know to be accomplished by a great variety of organs, eliminating different principles from the blood, as the kidnies, the skin, the lungs, &c. &c. Is it not rational, then, to suppose that the constituent principles of the blood are also furnished through different *routes*, and that they receive their specific characters from the peculiar action of the vessels through which they pass?

I would particularly call the attention of my readers to a very beautiful correspondence, which my hypothesis suggests between the extreme arteries and veins, and which certainly reciprocates much in favour of its truth.

All the exhalations and secretions we know to be effected

by the extreme arteries, or by vessels continued from arteries, after they have become of imperceptible magnitude. By them the materials of the system are recommitted to the control of physical laws. By the corresponding extreme veins, on the other hand, after being prepared in the stomach, they are taken up and go through precisely the reverse process, viz. assimilation and union with the vital mass. The egress of matter is through the extreme arteries, its ingress through the extreme veins. That the secerned fluids may not, in the descending gradation, bear away with them any of the still necessary principles, they are many of them passed through glandular structures; and that the aliment, in the ascending gradation, may not bear into the circulation any thing too little assimilated, it is made to pass through the liver, which in magnitude is equal to the aggregate of all the other glands in the system.

The analogy may be carried still further without violence. As there are some of the secerned fluids which pass out of the system through no other medium than the exhalant vessels, so there are nutrient fluids, which immediately enter the circulation through the lacteals.

It may still be objected to all which I have advanced on this subject, that the solvent powers of the gastric juice have been by many experimentors demonstrated out of the stomach, and that something like artificial digestion has been often accomplished. I am by no means desirous of concealing the magnitude of this objection; at the same time, I demand equal candor on the part of my readers, in relation to the experiments alluded to. In the essay, I mentioned the circumstances which would appear to render their results equivocal. As we are unable, either to define the precise change which the food undergoes in the stomach, or the properties of chyme, which vary with the varying diet, it is obvious that we have no standard to which these results are to be referred. It is easy to conceive that comminution and maceration of aliments in the fluids of the stomach, at the natural

temperature of the body, may effect such a change in their sensible properties, as that they shall resemble some varieties of chyme. It is possible, too, that the adventitious acids, which are very frequently present in the gastric fluids, may, in some instances, have aided the effect, and particularly in the decomposition of bones, for which but a feeble counter-affinity is required.

It would not be at all surprising, therefore, if with these opportunities for self-deception, something of this kind had been effected out of the stomach; particularly if the experiments were undertaken by one already fully persuaded of the truth of what he was only endeavouring to confirm. We know, from a thousand examples, that observations of this kind are exceedingly apt to be fallacious, when the experimentor thus anticipates the result.

But if such an effect from these experiments, even supposing that the gastric juice have no remarkable solvent properties, be not a matter of much surprise, their failure, *granting* the extraordinary chemical powers of fluid, would be one of very great astonishment, and particularly, if undertaken by those who had full confidence in a favorable result.

That these experiments have often failed, under such circumstances, in the hands of the ablest chemists, and without the omission of any circumstance calculated to facilitate them, is well known.

I have sought, in vain, for the original memorial of Montegre on this subject. I must therefore content myself with quoting Magendie's allusion to them, and to those of Reaumur.

"To establish, incontrovertibly, this point, it has been attempted to produce what has been called, since the time of Reaumur and Spallanzani, *artificial digestion*. For this purpose, after having masticated the food, it has been mixed with the gastric juice, and afterwards exposed in a tube, or other vessel, to the same temperature as that of the stomach. Spallanzani has asserted that they succeeded, and that aliments were reduced to chyme. But since the more recent researches

of M. Montègre, it appears to be shown that this is not the case, but, on the contrary, that the substances employed, did not undergo any alteration, at all analogous to that of chymification, which agrees with the experiments of Reaumur."

If, then, the results of these experiments be abandoned as equivocal by some of the ablest physiologists, and who, nevertheless, labour to prove the chemical solution of aliment in the stomach, it can not be regarded as presumption in me, if, after having adduced other circumstances to weaken their evidence, I also reject them.

The immediate coagulation of milk, by the secretions of the stomach, and by the mucous membrane of this organ, even after being kept for a great length of time, has been regarded as a strong circumstance in favour of the solvent powers of the gastric fluid; but is effectually answered by the fact, stated by Bichat,* *that all the mucous surfaces possess this property.*

A circumstance unnoticed in the essay, as favouring the common opinion on this subject, is the supposed discovery of chyle in the intestines. I do not deny that some physiologists† have detected a whitish fluid in the stomach and intestines, but they have by no means given sufficient reasons for identifying it with the fluid which is detected in the thoracic duct.

It will be found upon examination, that nearly all the characteristic propreties of chyle are predicated, exclusively, of that fluid which is found in the lacteal vessels, and that there are no facts, or experiments, which warrant the application of that name to the fluids of the stomach or intestines. This has arisen from not regarding sufficiently the assimilating office of the lacteals themselves. We have ample reason to believe that no fluids pass through capillary vessels without change.

The conclusions, then, which I would draw, from the ar-

* General Anatomy, v. 3. p. 82. † 2. Hunter, &c.

guments advanced in the essay, and in the present supplement, are:

First, *That there is no fluid secreted by the stomach, possessing the remarkable chemical properties, attributed to the gastric juice, and consequently that chemical solution does not take place in that organ.*

Second, *That there is a neuro-electric, or an electro-animal principle, conveyed by the nerves of the stomach, and made to act upon its contents through the medium of its saline secretions, which possess sufficient animal properties to facilitate its transmission, and to aid the effect.*

Third, *That the aliment in the stomach, thus undergoes what may be termed an animal decomposition, and after being reduced to its elements, is recombined by the principle which controls the composition of animal matter, though not probably endued with any properties of life, till it enters the circulation.*

Fourth, *That a very considerable portion of the aliment is absorbed by the capillary veins of the stomach and intestines, and consequently conveyed through the vena portæ, and the capillary system of the liver, in which route it undergoes still more important changes, and becomes endued with some of the vital properties of the blood.*

Fifth, *That another portion is taken up by the lacteal absorbents, by which it is converted into chyle.*

It may, perhaps, by some, be thought presumptuous in me. thus to attempt the refutation of opinions substantiated by numerous experiments and extended personal observation. If any apology be necessary, for the freedom which I have used in this discussion, I would observe, that whenever a fact is brought forward, for whatever purpose it may have been employed by the discoverer, it at once becomes the property of the literary community, and may be used for any logical purposes. It is generally the case, that experimentors, intent upon some preconceived hypothesis, take but a partial view of the relations which their results bear to the rest of the science. Although they contribute to the completion of the

structure, by bringing together the materials which nature has dispersed through her works, the scientific arrangement of them is usually reserved for other hands.

If I may compare small things with great, I would vindicate the course which I have pursued, by observing that nearly all the facts, relating to the circulation of the blood, were demonstrated to Harvey by his predecessors, and that his great merit consisted in drawing from them the correct inferences. It appears to me that a just arrangement of the established facts, in relation to digestion, is at present a greater desideratum than the multiplication of equivocal experiments. The materials are sufficient. The *present* arrangement is certainly incongruous. That a more symmetrical disposition of them has been my *object*, I trust there is no arrogance in avowing; of the success of the *attempt* the public will judge.

ART. II.—*Classification of Anatomy and Physiology.* By N. R. SMITH, M. D.

GENERAL ANATOMY.

ANATOMY.		PHYSIOLOGY.	
ELEMENTARY TEXTURES.		ELEMENTARY PROPERTIES.	
Chemically Constituted, and Organized.	Cellular Texture	Tenacity,	*Physical and Vital.*
	Osseous	Hardness,	
	Cartilaginous	Elasticity,	
	Fibrous	Flexibility,	
	Muscular	Smoothness,	
	Nervous	Extensibility and	
	Serous	Contract. of Tex.	
	Mucous	Sensibility and	
	Epidermoid	Contract. of Life	

ELEMENTARY *General Organs.*		ELEMENTARY *General Functions.*	
Resulting from union of Texture.	Blood-vessels,	Circulation,	*Resulting from union of properties.*
	Exhalants,	Exhalation,	
	Nutrients,	Nutrition,	
	Absorbents,	Absorption,	
	Nerves.	Sensation.	

COMPOUND *Independent Organs.*		COMPOUND *Independent Functions.*	
Resulting from union of textures and elementary organs.	Brain,	Moral & Intellect. Phenom.	
	Eye,	Vision,	*Resulting from union of elementary functions and properties.*
	Ear,	Hearing,	
	Palate,	Tasting,	
	Schneiderian Membrane,	Smelling,	
	Skin.	Protection, &c.	

APPARATUS *Of Associated Organs.*		ASSOCIATIONS *Of Functions.*	
Resulting from associations of distinct organs and textures.	Digestive Apparat.	Digestion,	*Resulting from union of functions and properties.*
	Respiratory ——	Respiration,	
	Secernent ——	Secretion,	
	Locomotive ——	Locomotion,	
	Sexual. ——	Generation.	

Nothing contributes so much to facilitate the acquisition of science, or to render the pursuit of it an intellectual enjoyment, as natural and lucid arrangement. The detail of insolated facts is always tedious and repulsive. It is in their relation to each other, as parts of a system, that they are important and interesting. The teacher who would excite interest in his pupils, must imitate the artist, who with a few strokes of the pencil, pourtrays the outline and general expression of the subject, so that in the filling up, each touch shall produce its effect in relation to the whole.

Classification must ever vary till the science, in which it is employed, becomes fixed upon demonstrable principles. It should always be predicated on our present knowledge. If an ingenious arrangement continue to be employed when, by the accumulation of facts, the materials of the science have become changed, it will retard our advancement. If it be formed in *anticipation* of the progress of our knowledge, it will almost always conduct our inquiries erroneously.

No man has contributed more, in physiology, to the accumulation of facts, or to the scientific arrangement of them, than Bichat. His system, however, with unequalled merits, possesses capital faults. I believe that neither have been

justly appreciated. His experimental analysis of the organs into elementary textures, and of the functions into elementary vital properties, lays the true foundation for the prosecution of the science on inductive principles. Even in this, however, there are certain inconsistencies which have diminished the influence which the method employed would otherwise have exercised. It will be recollected that, with the cellular, serous, mucous, fibrous and other textures, which are demonstrably simple, he associates the vascular system of red blood, of black blood, the nutrient, exhalant, absorbent and glandular systems. Any one, at all inquisitive, must, I think, be struck with the incongruity of such an association. Both classes of blood-vessels are obviously compounded, each consisting of three coats as distinct as those composing the stomach or intestines. They retain this compound structure till they become too minute for observation. We have strong analogy, then, for regarding, not only the most minute of the blood-vessels, but the exhalants and the nutrients which are continued from them, and indeed all the circulatory vessels, as compound. Bichat appears to have been led into this error, by the resemblance which they bear to many of the textures, in being generally diffused, not distinguishing between general *organs* and general *textures;* and also by the presumption that these general organs, the exhalants, nutrients, &c. are elementary with respect to the simple textures themselves. This, however, is refining beyond the reach of our senses. Those are to be regarded as distinct and simple textures which, so far as we can discern, are homogeneous in their structure. The vital and physical characters, *demonstrable* in them, do not depend upon the presence of the minute organs, of which we may suppose them composed, although their *formation* may, but are conferred upon the *texture*. But it is not certain even that vessels of *any* description enter the simple tissues. Their presence in them is presumed to account for their formation. May they not, however, be organized from the blood, by the neuro-galvanic in-

fluence, which is found to have so much control over the vital functions. If the latter has the least plausibility as a *presumption*, the former is nothing more. That there is some limit of this kind to vascular organization is very certain, for if, for instance, we suppose the cellular tissue to be vascular, we must suppose its elementary *vessels* to be also *vascular*, and so on to infinity.

His most important error, however, and which well illustrates the evil resulting from hypothetical arrangement, is his division of the functions of the individual into animal and organic. There certainly can be no impropriety, when tracing the gradations of life, in designating those functions as *organic*, which constitute a being, as for instance a plant, that has no discriminating perception of surrounding objects; or in denominating those *animal functions*, which, being superadded, establish this last interesting relation. But when, from regarding these classes of functions as distinct in two classes of beings, we attempt accurately to distinguish them in an *individual*, we are involved in difficulty, for the latter are so engrafted upon the former, and the two hold such intimate intercourse through important common organs, that we cannot discover any natural demarcation, nor determine what degree, or kind of influence, one exerts over the other. This feature of Bichat's classification was predicated on an anatomical and physiological distinction, between the sympathetic and cerebral systems, which has since been shown to be hypothetical. We have already observed that classification should, unless professedly artificial, be founded only on established principles, for we employ it merely for the purpose of embodying our facts, and by no means to conduct our investigations, since such an order would be altogether inconsistent with *inductive* inquiry.

Neither is there a proper symmetry between the two primitive classes of this author. I mean, 1st, the functions relating to the individual, and 2d, those which are necessary to the species. The whole of the latter class is not of greater mag-

nitude and interest than some of the subdivisions of the former. It is furthermore objectionable as being unnecessary. No perplexity need arise from considering the function of generation as belonging to the individual.

Another objection to the above classification is its being so minutely and speculatively ramified, that, instead of presenting to the student at one view, a clear outline of the *subject*, it is itself a source of perplexity.

It will be seen that, in the foregoing table, I have endeavoured to embrace the excellencies of Bichat's classification, whilst I reject so many of its principles, deemed erroneous, as to give it almost entirely a new aspect. It has been my object to carry through the whole science the anatomical and physiological climax of composition, which is suggested by his elementary textures and elementary properties.

With regard to the distinction of elementary textures, I would observe, that Bichat's system has been criticised by many who have not appeared to understand the principle upon which it was founded. It was not his object to resolve every part of body into the ultimate constituent principles, to which they may perhaps be reduced by maceration and other mechanical means; but to designate those as elementary, in relation to the organs, which are, in their *natural state*, characterized by peculiar physical and vital properties, and which are uniform in their appearances and offices in every part of the system. Thus, the serous membranes may be resolved into cellular tissue, but they are strongly characterized by peculiar properties, which are conferred upon this particular condition of that tissue, and are to be regarded as distinct. The same may be said of the bones. They have erred, therefore, who have endeavoured to reduce the simple textures of Bichat to the number of three or four. There are some, however, which may with propriety be rejected as mere *varieties* of other textures, as for instance the medullary, the pilous, &c. Certain compound textures as the fibro-cartilaginous, in which the properties of the constituents are

not changed but merely modified, need not be treated of distinctly.

It will be seen, that, in my arrangement, the blood-vessels, absorbents, nerves, &c. with their functions, constitute a class by themselves. The name which is conferred upon them expresses the reason for this disposition. They are compound, and they are general, all of them entering into the composition of most organs, and some of them supposed to be essential to all. The functions which they perform are general, and for the most part, common to all the compound organs. Although I would in the first class, treat of the nervous *texture*, yet the *nerves* are to be regarded as elementary organs, bearing a relation to the system, corresponding to that of the blood-vessels, &c. They are also compound and divisible into nervous and cellular textures.

The next class consists of compound independent organs, and their corresponding functions. It cannot be supposed that I would mean absolutely independent, but merely that each one of these organs completes its function singly, without the aid of subsidiary organs. Of course they are connected with the system, through the medium of the elementary general organs, of which, and of simple textures, they are compounded. The phrase which I use to designate this class, will be better understood when regarded in contradistinction to that by which I denominate the fourth and last class. In this it is obvious that several distinct organs contribute their several functions to one common function. I therefore denominate the association of these organs an apparatus, and the result of their united offices an association of functions.

It appears to me that the facility and the interest with which anatomy and physiology might be studied, are much diminished by the unnatural separation of these two parts of the science, which at present prevails. The structure of an organ is only interesting in relation to its function, and its function is only intelligible by reference to its structure. The plan which I have pursued, it will be seen, recognises this principle.